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ARTICLE 1.

STRANGULATED HERNIA, TREATED BY OPIUM.

READ BEFORE McLEAN COUNTY MEDICAL SOCIETY, OCTOBER 1857, BY S. W. NOBLE, M. D., AND PUBLISHED BY RESOLUTION OF THE SOCIETY.

GENTLEMEN: When I received an appointment at your hands to read an essay on this occasion, I determined to write on some subject relating particularly to the practice of our profession. And while contemplating in my own mind what subject to select for discussion, I was called to treat a case of Strangulated Hernia, which reminded me of some experience I have had in the treatment of that disease which I have long thought of presenting to the consideration of this Society.

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I hope, therefore, no apology will be necessary for departing from the course of my predecessors on such occasions, and presenting a simple relation of facts, as I have observed them, in the treatment of a few cases of Strangulated Hernia. It is not expected that we will go into a full description of the nature and causes of Hernia in a paper of this kind; neither is it necessary, for the disease is so plainly described in every work on surgery, that it is expected of every practitioner that

he will recognize the presence of a Hernia so soon as it becomes strangulated.

What I propose, then, for discussion to-day, is the indications of cure which present themselves in the treatment of a case of Strangulated Hernia, and the means by which these indications may be fulfilled.

The first and principal indication of cure is to relieve the stricture and return the Hernia, which is to be accomplished, if possible, by taxis, as directed in the books. But if the taxis, aided by reliable auxillaries, fail, I believe that all authors, so far as I have read, lay it down as a rule, and urge the importance of the practice, that as soon as the surgeon is satisfied that the strangulation cannot be relieved by the taxis, he is bound to operate, and that without delay, as the only chance of relief. And the great majority of experienced surgeons agree that in regard to this operation, error is more frequently on the side of delay than of precipitancy.

"Two circumstances," says authority, "demand its instant performance: a conviction that by no other means than the edge of the knife, directly applied, can the abdominal outlet be so enlarged as to relieve the constriction and admit of replacement; also a conviction that already inflammatory action has advanced so far that either ulceration or sloughing is inevitable in the protruding parts. In the one case they operate to relieve the stricture—effect replacement hoping to arrest inflammation. In the other they operate to relieve the stricture, and leave the hernia unreduced, and prevent fatal extravisation of intestinal contents within the abdomen."

In the face of the above, and long established convictions, a man would be considered reckless indeed, who would for one moment hesitate to operate, or would try other and milder means to produce the same effect. Were it not for the old maxim: "That the true science of surgery consists in relieving surgical cases without the use of the knife," I should not presume to offer any objections to knifing a hernia; neither will I attempt to offer a substitute for the knife in all cases of strangulation. But while the watchword of the age is progress, I don't think it impossible for us to make some change, at

least, if not improvement, in this long established course of practice.

And when we take into consideration the mortality attending the operation, which is admitted on all hands to be great, it is not to be wondered at that we should hail with pleasure any means calculated to reduce the amount of mortality from that cause. This is particularly interesting when we reflect for a moment on the frequency of the accident or disease. I have seen it estimated by some writer in a late journal, that one out of every fourteen persons in the community have a hernia. This proportion, when viewed in connection with the universality of the disease, would show that a larger number of the human family are affected with it than with any other disease that flesh is heir to.

The common disorders of life are, for the most part, limited to particular portions of the earth's surface; some limited to particular seasons, some to particular races, some to particular classes of the race, and some to certain periods of life; but hernia is not subject to any of these limitations. The American and the Asiatic—the infant and the octogenarian—the citizen and the backwoodsman, are all alike subject to it. It may be said of the unfortunate class so affected, that they stand in jeopardy every hour; they know not but the next step or cough, or slight irregularity will induce strangulation that shall prove fatal or otherwise, according as the physician in attendance may happen to be skillful or otherwise.

The treatment that we propose for the relief of Strangulated Hernia is full and oft repeated doses of opium until complete narcosis is produced. When given in this way opium will evidently produce a more complete relaxation of the muscular system than any other known remedy. It possesses this advantage over most other relaxing medicines: you can manipulate without giving pain. But, on the other hand, if you relax the system perfectly with tartar emetic or tobacco and apply the taxis, you cause pain in the parts which excite contraction in the muscles implicated in the hernia, which are the very ones you wish to relax. The same objection applies to chloroform, with this slight exception: the patient is not

conscious of the pain, and you have not the influence of volition to contend with.

Opium, likewise, when given in large doses; say from 3 to 5 grains to an adult; arrests vomiting, which is one of the most unpleasant symptoms, quiets the irritability of the system, and effectually prevents inflammation and mortification in the strangulated portion of the bowel. When the patient is thoroughly and permanently under the influence of opium, if the stricture is not relieved and the bowel returned spontaneously, it may be readily accomplished by a renewal of the taxis. At least that has been my experience in these cases.

I have not arrived at these conclusions through the channel of any fine spun theory, but have given this subject much thought and study, and if, after hearing the result of the cases treated in this way, which I am now going to report, the members present are satisfied that I am in error in recommending this course, or that the practice would be unsafe and empirical, and will show satisfactory grounds on which they base their convictions, I will agree, in the next case of Strangulated Hernia that I treat, to send for a surgeon, early. Bnt while I am in the same state of mind I am to-day, I shall never resort to the knife to relieve stricture in those cases.

My thoughts were first directed to this subject strictly by accident, though I have since seen in some of the journals (that I have been unable to lay my hand on while preparing this paper) that an English surgeon recommended the opium treatment in cases of irreduceable hernia, to fill the same indications.

I will now give a report of the first case treated in this way, which will fully explain why I first ventured to try the opium treatment, while the result of the case will be ample excuse for trying it in the next case.

Called, March 7th, 1850, at 4 P. M., to see H. B., (known to most of you,) whose hernia had become strangulated about noon of the same day, which caused severe pain in the bowels, constant retching and vomiting of stercoraceous matter—in fact, all the symptoms of Strangulated Hernia in

their aggravated form, which had not been relieved at all by an irregular, who had been treating him for the two hours previous to my visit, as the "most desperate case of cholera morbus he had ever witnessed in a thirty years' practice." I placed the patient in position, and applied gentle taxis for a few minutes without relief. I then gave nauseating medicines to relax the muscular system and applied cold to the tumor, with no better effect.

Being fresh from the schools and fully impressed with the importance of an early operation, I very candidly informed the patient and his family that there was no hope of relief without an operation, and urged the propriety of its immediate performance, after fully explaining the danger attending the operation.

The old gentleman readily consented to submit to whatever course I thought best, but was inclined to think I was too young to use the knife with safety and proposed sending to Bloomington for an old physician. I expostulated with him for awhile, giving it as my opinion that, owing to the condition of the roads, which were almost impassible at that hour of the night, it would be too late for council to be of any avail in his case before he could procure it. Seeing, however, that he was determided in his course, a messenger was sent in haste for Dr. Thomas, who was at that time supposed to be the oldest physician in Bloomington. The night being unpleasant, Dr. Thomas refused to turn out, but told the messenger to hurry home and tell me to give the patient a big dose of morphine. By the way, we would here remark that we have since learned, although we did not know it at the time, that the Dr. intended his morphine as a curative instead of a palliative. That he has treated hernia in this way. The messenger being satisfied that there would have to be an operation performed, was not willing to return without council, so he called on Drs. Colburn & Rogers (our venerable President) who were soon enlisted in the spirit of the case and gathered up their knives, a bottle of ether and a copy of Gibbons' Surgery, and were at the seat of action by 2 o'clock A. M., when the same course of manipulation was

again gone through with, after relaxing the system thoroughly with an injection of tobacco, without being able to reduce the hernia. I had given a pill of opium when the messenger started, which had quieted the irritability of the stomach for a time, but the effect had now entirely worn off, and the symptoms continued severe, with a decided increase of pain in the tumor.

The patient and friends were again assured that there was no hope of relief without an operation, which was again explained to him, and the dangers frankly admitted. Consent to an operation was given.

Day was dawning, and as we were all tired from our night's exercises, we agreed to take breakfast, which was nearly ready, and arrange matters for operating as soon as it was light. After breakfast we cut our adhesive strips; threaded our needles; constructed an operating table; got the patient on it, and partly under the influence of ether. I had the tumor in one hand and the knife in the other, just ready to distinguish myself as a surgeon, when the patient cries, "Hold! I fear I may die, and should like to see my wife once more." We tried in vain to persuade him out of this notion, but he plead so strong we finally agreed he might have a parting word with her. Now it is to be regretted that these old people did not live very happily together, hence it is not to be wondered that when the old lady was ushered into his presence she did not show signs of the most heartfelt sorrow at the prospect of being so soon left a widow. Be this as it may, when he saw her, says he: "Stop! take away your knife; I will die and be out of her way." With this he crawled from the table, and re-arranged his bed, and all we could say or do we were unable to persuade him back to the table. We plead with him as we would for his life, believing as we did at the time, there was no salvation for him without the knife. We likewise thought there was a strong probabiliy of saving him by an operation. Finally we gave it up. He said that he wished to die, and all he wanted of us was to give him something to die easy on.

After a very grave consultation, it was agreed to give him

morphine enough to relieve him of all suffering this side of the grave. We gave a large dose, something over a grain. We then took a very feeling farewell of our old friend, and Dr. Rogers administered what spiritual consolation he could under the circumstances, and left for our several homes. He took his morphine, turned over and went calmly to sleep. In the evening, about 5 o'clock, I was passing the house, and seeing no bedstead sitting out, or other signs of a funeral, I ventured in to see how he was doing. I found him lying on his back, presenting more the appearance of a cadaver than any thing else, breathing about eight times per minute, pulse forty, full and regular, skin covered with a cold, clammy sweat, muscular system perfectly relaxed. I thought from appearances he would die, and that soon, from the effects of the morphine, if from nothing else. But I concluded that I would again try to reduce the hernia, when, to my utter astonishment, there was no obstacle at all in the way of replacing the bowel. I recommended the cold douche and strong coffee, to relieve the system from the effects of the morphine, but still thought he must die sooner or later from gangrene, according to the books, judging from the length of time the hernia had been strangulated, so I did not venture to call upon him again for two days, when I found him sitting up quite comfortable, and very much inclined to laugh at the uncertainty of human prognostications.

He convalesced rapidly, and lived to give his family and the county authorities no little trouble.

Case II.—The next case of hernia that I saw was in 1852, in a boy 2½ years old. Had been troubled with a scrotal hernia for six months. The patient was much reduced by a protracted attack of ague. His hernia became strangulated and was treated three hours as a cholic; but owing to the severity of the symptoms, and the fact of his throwing up fœces, the parents became alarmed, and I was called in some four hours from the onset. I thought it was an unfavorable case for an operation; besides, by this time I had myself pretty well persuaded that the opium treatment was safe and reliable. So I concluded, after an ineffectual effort to replace

the bowel by taxis, to give the opium a fair trial in this case. Accordingly I prescribed opii. gr. 1, every two hours, until he was thoroughly narcotized. Being fatigued, I laid down with directions for them to call me as soon as he slept soundly and could be moved about without waking. His stomach retained the medicine, and I was not called until after he had taken the third powder, when, to my surprise, on examination, I found the hernia had entirely disappeared. Patient slept till morning, when his bowels were moved by enema. Quinine was prescribed for his ague, and a truss supplied for the hernia, but the ring was so sore from our manipulations that he could not wear the truss. Strange to say, the bowel has never descended since, and to all appearance the hernia is radically cured, though we do not attribute the radical part of the cure to the opium.

Case III.—Though not successful, served to convince me more fully of the importance and reliability of the opium. I report it cheerfully, notwithstanding it is very uncommon for physicians to report unsuccessful cases:

June 1st, 1854. My friend Dr. Craig requested me to visit a patient of his whose hernia had become strangulated about six hours previous to my visit. The Doctor had tried the taxis, aided, I believe, by the whole list of auxillaries, without being able to reduce the tumor. The patient *pointedly* refused to submit to an operation. I then suggested the opium treatment, which the Doctor readily consented to, and we ordered ½ gr. morphine, with a little soda, every hour, until narcosis was produced, when the Doctor was to be notified. But unfortunately for science and the man's own welfare, the family were not strictly honest with us, and when the man began to roll up his eyes and see sights, they put the powders aside, and told Dr. Craig, on his next visit, that the patient had taken them all, but had thrown them up. The Doctor repeated the prescription with the same relief to the pain and vomiting, but was unable to narcotise him thoroughly for two days, when business called him from home. He left orders with the family for them to send for me immediately; but they were so strongly prejudiced against the use of opium,

and the man was resting so easily, that I was not sent for until late in the evening of the 26th, when there was a return of all the symptoms, with a decided increase of pain and soreness in the tumor. I prescribed morphine, gr. 1, every hour, and calculated to stay and see why it was that he would not become narcotised, but was called away and detained until morning. When I returned I found he had rested well, and they reported him to have taken three doses of the prescription. This I doubted, and directed 1 gr. morphine every hour, and stayed to see that they were given.

In about one hour after he took the second powder I succeeded in reducing the hernia without any trouble. He drank a strong cup of coffee and got up to the stool and had a very copious evacuation from his bowels, but died that evening, evidently from gangrene, though there was no post mortem allowed.

This man's life, I verily believe, might have been saved if they had been more honest with the Doctors.

[Cases IV, V, and VI, were all cases of Inguinal Hernia, in male adults, relieved by the taxis within eight hours from the first prescription of opium. The treatment and results were so near like cases I and II, that we deem it unnecessary to cumber the pages of your journal with an extended report.—Sect.]

Case VII is one of unusual interest, from the length of time the stricture remained, the amount of opium taken, and the favorable termination. It shows in an eminent degree, the power of opium in preventing inflammation and mortification. It likewise demonstrates what a degree of tolerance for opium is established or created by severe pain. One gr. of opium is a full dose for the patient under ordinary circumstances, yet under the influence of pain he took, in thirty-six hours, seventy-two gr. with impunity.

Called, July 22d, 1857, to see G. D. C., but being indisposed at the time, my friend, Dr. I. M. Coleman, kindly consented to visit him for me. He found a large scrotal hernia strangulated. After trying all the usual remedies and manipulations without being able to relieve the stricture, at

my suggestion he put him on opium, gr. ii. every hour. He took the prescription all night, but owing to the severe pain and irritability of the stomach, (some of the opium being thrown up, no doubt,) he was not narcotised. Prescription was continued twenty-four hours longer without any perceptible effect on the system except the partial relief of pain and soreness. Vomiting was frequent, but less stercoraceous. I was now (the 23d) able to visit the patient, and Dr. Stipp, of Bloomington, was added to the consultation. Dr. Stipp was in favor of an operation but thought it too late to be of service. At his suggestion we introduced a long flexible tube, and filled the rectum and colon with warm water, which passed away without fœces. We then injected laudanum up the rectum, but it was not retained. The case now was considered almost hopeless, but I was anxious to try the opium further, so we agreed to give him sulp. of morphine, gr. 1, every hour, until he was narcotised. His stomach rejected the first dose, when a second was given immediately, and I remained with the patient to watch the effect. After taking the fifth portion of morphine, the patient went to sleep. In thirty minutes more I succeeded in reducing the hernia with the greatest ease. He was thoroughly narcotised, and the muscular system perfectly relaxed. We gave strong coffee freely, moved bowels by enema, and in sixteen hours he was able to be up. He complained of but little soreness or inconvenience about the ring, notwithstanding the tumor had been "worked with" frequently and sometimes severely, for the last sixty-five hours.

Convalescence was rapid, and in four days he resumed business.

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We are aware that the number of cases here reported are insufficient to establish any one principle in the practice of medicine. But the report contains all the cases of strangulated hernia that have occurred in my range of practice since 1850. The result of the treatment we deem sufficient apology for presenting the subject for your consideration. Hoping the members will give it a candid consideration, and

a fair trial when opportunity offers. If my conclusions are correct, that opium prevents inflammation and gangrene, you are running no risk, for if you fail with this treatment you can still resort to the knife, and with less danger of the operation being followed by a fatal peritonitis.

There may be nothing essentially new in this mode of treatment, but if it has ever been recommended or practiced in this country I am in "blissful ignorance" on the subject. It certainly is not spoken of in our text books or in any medical literature that has come under my observation.

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Since the above paper was read the opium treatment has been tried by several members of our society. Dr. A. H. Luce, of Bloomington, reports two cases treated in this way with entire success, and Dr. Harrison Noble, of Keyworth, one. Dr. I. W. Coleman, of Le Roy, has furnished me with a full report of a case treated by himself and Dr. T. D. Fisher, an abstract of which I furnish for publication as a continuation of the essay above.

Called April 1st, 1859, at 3 o'clock P. M., to see Mr. A. C., age 55, whose hernia had become strangulated the evening before. Found him suffering much pain, with retching and vomiting of stercoraceous matter. I placed him in situation, and endeavored to reduce it by taxis, but failed, after repeated trials. Having been associated with Dr. S. W. Noble, in the treatment of one of the cases reported in his paper, I at once resolved to resort to the opium treatment, having faith that it was equal to the emergency. Prescribed opii. gr. i, every hour, until he should become narcotised, and left, promising to visit him early next morning.

Being unavoidably detained, I did not get to see him till noon of the next day. Learned that patient had slept soundly during the night, and on waking at six in the morning, much to his joy, his hernia had returned. But having occasion to pass his water, while standing over the chamber, his hernia came down again. The pain and vomiting having returned, we a second time put him on the opium, and left him. Next morning, at day-break, we were summoned in haste to see

him, the messenger reporting him decidedly worse. Dr. T. D. Fisher, his family physician, having been absent and returned, visited the patient with me. Patient was much exhausted from loss of sleep, pain, and vomiting, and on inquiry we learned that the friends, after giving him one of the powders and his throwing that up, concluded that it was the medicine which caused so much irritability of the stomach, and did not give any more.

After consultation we concluded to give morphine, gr. i, and wait awhile, and then try the taxis; but did not succeed in the reduction of the tumor. Cold water, ether, and enemas were tried, with no better result.

Not yet discouraged, we decided to give the opium a fourth and last trial. Prescribed morphine, gr. i, every two hours, until narcotism came on, and left, promising to be back in the evening. On our return we found the patient comfortable; vomiting had ceased, and the tumor was materially reduced in size. The bowels having been moved, taxis was again applied, and the hernia was replaced without difficulty. Convalescence was rapid.

Le Roy, Ill., Feb. 13, 1860.

ARTICLE 5.

For the Chicago Medical Journal.

PHYSIOLOGICAL INCEST.

BY W. BOYD POWELL M. D., COVINGTON, KY.

Messrs. Editors:—By the above title I designate marriage in contravention of physiological law.

There obtains in society a very general conviction that consanguine marriages deteriorate the species by entailing on progeny idiotcy, other mental imperfections and constitutions so depraved as readily to develop scrofulous forms of disease; and yet it is strongly to be suspected that the truth of this

general conviction has never been established. Some fifteen years ago I was induced to suspect the existence of a physiological incompatibility between the sexes in relation to progeny and thus I was induced to adopt a course of action and careful observation of progenitors and progeny, and the result has been a thorough conviction that my suspicion was well founded,—that a physiological incompatibility in relation to progeny obtains very frequently between the sexes, and it consists in this:—certain of the human temperaments are incompatible in relation to progeny. This *a priori* appears to be much more probable than that consanguinity should produce an incompatibilty, because temperament is a positive condition and therefore may become an agent; but consanguinity is merely a relation which can exert no agency.

I nominate this physiological incompatibility, physiological incest; and to the extent of my knowledge it is the only incest nature has established; and to this incest do I attribute all the idiots, much of the insanity, all genuine phthisis, and all scrofulous forms of disease, half of the juvenile mortality, and a large percentage of the premature adult mortality which now afflict the human race. I admit however, that these evils do attend occasionally consanguine progenitors, and as consanguine parties are not physiologically exceptions to the race, it follows that they are as vulnerable to physiological incest as other parties, but I deny that consanguinity has any agency in this relation.

That system of the temperaments which I adopt, with some modification, is that which has descended to us, probably from Hippocrates or Galen. It embraces four temperaments, namely: the Sanguine, the Bilious, the Lymphatic, and the Melancholic. The description which has been had of the Melancholic so strongly indicates a pathological condition that physiologists seem to have, by a common consent, ceased to regard it as a physiological one; nevertheless, early in my investigations of the temperaments, I became convinced that humanity was distinguished by four constitutional conditions or temperaments, and after seven years of unceasing observation, I discovered an individual whom I supposed to

represent the fourth temperament, and I nominated it the Encephalic, and no fact has since come to my knowledge to induce me to suspect that I had adopted an erroneous conclusion. But I strongly suspect that the ancients observed the same condition and nominated it the Melancholic, but if they did they left no evidence that they comprehended either its anatomical or physiological condition. The Encephalic temperament is distinguished by a large cerebrum and a small cerebellum, consequently all the functions incidental to life, both animal and vegetative, are feebly manifested.

The four elementary temperaments, by their combinations, produce ten more, making, in all, fourteen, and for the purpose of convenient reference, I will tabulate them.

THE ELEMENTARY TEMPERAMENTS.

1. Sanguine temperament.
2. Bilious "
3. Lymphatic "
4. Encephalic "

THE BINARY COMBINATIONS OF THE TEMPERAMENTS.

5. Sanguine bilious temperament.
6. Sanguine lymphatic "
7. Sanguine encephalic "
8. Bilious lymphatic "
9. Bilious encephalic "

TERNARY COMBINATIONS OF THE TEMPERAMENTS.

10. Sanguine bilious lymphatic temperament.
11. Sanguine bilious encephalic "
12. Sanguine encephalic lymphatic "
13. Bilious encephalic lymphatic "

QUARTERNARY COMBINATION OF THE TEMPERAMENTS.

14. Sanguine encephalic bilious lymphatic temperament.

Application of the Temperaments to the Elucidation of Physical Incest.—All the temperaments are incompatible with themselves, but 1, 2, and 5 are less so than the others; and an alliance between either two of 1, 2, and 5, is incompatible; but 1, 2, and 5, are compatible with all the others.

But no other than a highly incestuous marriage can be made between any two of 3, 4, 6, 7, 8, 9, 10, 11, 12, 13, and 14.

If an alliance between 3 and 4 produced any progeny, which I think to be very improbable, it would be dead born. But such a marriage I have never seen.

Death in infancy attends the progeny of an alliance between persons indicated by 6 and 7. And an alliance between persons indicated by 7 and 8, is equally unfortunate.

The progeny of an alliance between 7 and one of same, will die in infancy of hydrocephalous, or tubercular meningitis.

The progeny of progenitors indicated by 8 and 9 will die young of scrofulous forms of disease.

The progeny of progenitors indicated by 9, will be idiotic.

The progeny of progenitors indicated by 9 and 14, will, some time in early life, become insane.

The progeny of progenitors indicated by 8 and 11, will die of tubercular disease of the lungs or mesenteric glands.

Two-thirds of the progenitors indicated by 13 will be dead born, and the other third will die during infancy.

Progenitors indicated by 12 and 13 will be attended with similar results.

P. S.—I have given so much attention to this subject, that I can at sight distinguish physiologically incestuous parties, and can teach any other person of respectable capacity to do the same.

Now, Messrs. Editors, do you entertain a doubt as to my having made the discovery herein claimed? If you do, review your own practice for a few minutes, and you will be able to call up cases that will sustain me. Have you not treated scrofulous forms of disease in the families of sound and healthy progenitors, those, too, whose constitutions contra-indicated any scrofulous taint? Such is the depopulating influence of this incest, that were emigration to our country to cease, I believe it would, in two hundred years, depopulate our country. If this contribution find a place in your journal please favor me with a copy.

I will conclude by giving you a patent fact. Dr. Patterson, Superintendent of the Idiotic Asylum at Columbus, Ohio, in response to my inquiries, says that he has carefully inquired as to the progenitors of the idiots brought to the institution, and has not been able to learn that more than two per centum of them were of consanguine progenitors.

ARTICLE 8.

STOMATITIS MATERNA.

BY L. S. ELLIS, M. D., CHICAGO, ILL.

This disease has, of late, elicited much attention from the medical profession. Its severity and obstinacy have been not a little magnified; its pathology pushed far into the mysterious; its treatment as varied as speculation, and speculation verged near upon absurdity. It has been represented as a new disease, which the oldest practitioners "have never yet met with," and a recent writer "could not find a single reference to this disease in any of the works of Practice, or Special Diseases of Females."

Dr. Dunglison, in his Practice, printed in 1842, vol. 1, p. 31, says: "It is not common with us, but it is said to be very often met with in moist countries, as in Holland, where it reigns at times epidemically, and is a serious affection, attacking adults, and child-bed females especially." Again, on p. 32, "The author has observed some obstinate cases in women who were nursing, the affection appearing to be induced by the constant drain from the mother interfering with nutrition."

In Bell & Stokes' Practice, 1845, I find, "There is yet a kind of sore mouth, which, as far as our present knowledge extends, is seen only in women during lactation." Vol. 1, page 52.

Dr. Wood gives the disease as extended review, as the relative importance of the disease demands.

The medical literature of this malady is more abundant, and in our country, extends back at least thirty years. I have not yet seen any such evidence as to induce me to believe that the disease is of modern date. The great source of modern improvement in medicine consists in a more accurate isolation and discrimination of particular disease. This accurate discrimination of disease alike tends to the advancement of our profession, and to individual success. In many cases what were considered symptoms by old authors, are now exalted into the ranks of distinct diseases. Also, by reference to authors as old as Cullen, we find whole groups of distinct diseases grouped under one head or class, determined by locality. Thus, under the title of Hepatitis, Cullen gives but three pages to diseases of the liver. While thus he makes no mention of such diseases as *fatty degeneration* or *cyrrhosis*, no one will claim that such maladies had no existence in his time. I believe that wherever a like combination of influences exists, there like consequences will follow.

Views of the pathology of this disease are as diverse as the localities of the authors. A recent writer says, "In all these cases I have seen inflammation of the cervix uteri and of the superior portion of the vagina." Another finds the *causa belli* in "miasmata," in "biliary derangement," and in "the influence of gestation and lactation." Another dives deep into that medical arcana, the blood. That inner temple of mysteries holds a wondrous amount of vagaries, whose warrings, if visible, would reveal to us a terrible pandemonium in that "liquid flesh" of ours.

The most positive testimony exists as to the unlimited extent of the prevalence of the disease. Proof is as abundant as of any disease whose history has been so recently written. None will doubt its existence in Illinois. Prof. M. M. Pallen speaks of it in Missouri; Dr. Shanks in Tennessee; Dr. Brandon in Georgia; Dr. Armor in Ohio; Dr. Dunglison in Pennsylvania; Dr. Backus in New York; Dr. Hale in Mas-

sachusetts; Dr. Marshall Hall in England; M. Guersent in France and in Holland. Names could be cited by scores, showing that the disease is not confined to malarious districts of our country. It occurs alike on the mountains of New England and New York, as well as on the prairies of Illinois. It exists in England, where Dr. Watson says, "Intermittent fever is known to us only in its group of symptoms." Likewise on the continent its existence with malaria exclusively is without proof. Before we accept this dogma of its malarious relation, we ought certainly to have some *positive* proof of its obeying malarial laws; proof that it occurs in greatest extent and severity at the same seasons; decreases in cities; vanishes with the settlement and cultivation of our country, instead of with the "*early healthy physical developement* of the daughter." On the contrary, I have observed it at all seasons, following not in the channel or seasons of epidemic malaria, but after fortuitous occurrences with those whose idiosyncracies predispose to it.

As before intimated, the *blood* has been obliged to assume the burden and responsibility of this disease. As a scapegoat of physical aberration, it has borne, not only the disease in its obscurity, but its task-masters into still greater mysticism. In this connection I cannot forbear quoting from a recently promulgated theory, especially illuminating:

"Of its dependence upon the depravity of the blood, I have no doubt. I consider the health of this vital fluid dependent upon the harmonious action of all the assimilative organs of the body, and a due supply of *pabulum* of proper blood food.

"If any one of the tissues fail to be nourished by an error of nutrition, I think the elements of that tissue *remain* as abnormal constituents of the blood; and that the elimination of those particular elements by their natural elective power, *alone* can render it normal for the production of the other tissues.

"The developement of *hair* upon the *fœtus* in utero, is in obedience, I think, with *this* general law. We know of no other physiological utility, as it almost always, very soon after birth, gives place to a reproduction. If its elementary con-

stituents are eliminated in the progress of fœtal development, that the *blood*, after this assimilation, might be in its integrity for a second assimilation to nourish another tissue; and the succession of these assimilations maintain not only the proper constituents, but the constant normal catalysis of the *blood*, then we see how necessary are the developement of all the uses *in utero*, as well as after birth. And the development of rudimentary structures in the *one sex* when the requirements of organs are confined to the *other*, as in case of the mammary gland, etc."

To know what the above means, what are its relations to Stomatitis Materna, and how it proves it a blood disease, the inquirer must refer to Trans. Ill. State Med. Society, 1859, p. 43, et. seq."

It is "a blood disease?" If so where is the proof? Do the speculations of writers make any approximation to proof? Does the successful therapeutical management of the disease, even by such theorists themselves, afford any support to their theories? If any knowledge of practical value is to result from theory, it seems it ought, in some manner, to be supported by microscopical investigations, corelative symptoms, and therapeutical applications. Dyspepsia may be accompanied by a "paucity of blood corpuscles." Is it, therefore, a blood disease? Prolonged lactation will favor "impoverishment of blood, impair nervous agency, and derange the secretions." Is it a blood disease? Why not come to the obvious and palpable conclusion, that the stomach is primarily at fault—that the "impoverished blood," the "deranged biliary secretions," the "constipated bowels," the "irritated stomach," the "acid urine and saliva," and the ulcerated tongue, are but symptoms of the central derangement—are but indices of impaired digestion, not in the nursing woman alone, but in the dyspeptic man and child.

In all cases falling under my notice, I have found evidence of impaired function of the stomach. This will be found a leading characteristic of the malady if attention is directed to it. These functional derangements do not follow as a sequence of the stomatitis, but are found to precede it in many cases a

long time. Women who have had this disease many times, learn by experience to avoid indigestible substances, especially such as induce acidity of the stomach. I have observed connected with this disease, loss of appetite and taste, acidity of stomach, gastric uneasiness after eating, constipation or diarrhœa, persistent or alternating, biliary derangement &c. Following these as a sequence in long continued cases, comes in a long train of "diminution of blood corpuscles," "anemia," "land scurvy," modification of condition of blood," &c., of various authors.

Rapid decay of the teeth is also apt to occur in this train of symptoms; a circumstance I have always referred to the acid secretion acting on the teeth. Herein is the explanation of so many American women losing their teeth, especially in the first lactation. This seems to me to be as much entitled to a place in the category of blood diseases as Stomatitis Materna. If the above described condition of the stomach continues unabated, the decay of the teeth is almost sure to occur, more sure than the disease under consideration.

I am not alone in this mention of gastric derangement. Dr. Dunglison, in his Practice, vol. 1, p. 31, says: "This form is accompanied by great cephalic, gastric, and general disturbance, and at times the eruption appears to extend to the intestinal canal, giving rise to severe pain in the abdomen, diarrhœa, and typhoid symptoms, under which the patient may sink."

Dr. J. H. Hollister says: "In a great number of instances the disease has made its appearance as the immediate sequence of constipated bowels, and was relieved almost as soon as the proper correction was made." Vide Trans. Ill. Med. Society, 1859, page 47.

Dr. D. S. Brandon, after enumerating the mouth symptoms, says: "To these may be added, burning in the stomach, with occasional vomiting, constipation, or diarrhœa, more or less obstinate." Southern Med. & Surg. Jour. Jan. 1860, p. 4.

I am now in attendance on Mrs. R., who is seven months in pregnancy. She complains of acidity of stomach, and evidences of gastric derangements, and the urine is so intensely

acid as to produce severe inflammation and excoriation of the external parts. She has had Stomatitis Materna during previous lactations, both here and in Central New York. I look for the same sequence after her confinement, unless that condition of the stomach is relieved, but not otherwise. In one of the worst cases I ever saw, Mrs. H., it always accompanied acidity and was readily relieved by tonics and antacids. I could point to case after case presenting the same association of symptoms, readily relieved by remedies addressed to the stomach, so that I am accustomed to direct my attention to that organ, and am not inclined to look upon the disease as so uncontrollable and unpromising as many writers.

Much of our knowledge of the *character* of disease is derived from observance of the *effects* of remedies. An important hint, as to the pathology of this disease, may be obtained by reviewing the various remedies proposed by different writers. Dr. Backus may have been hypothetical in his opinion of this malady, but he certainly indicated a principle in its treatment, the wisdom of which is evidenced by its general adoption. His prescription is as follows:

℞—Carb. Ferri.		grs. xlv.
Pulv. Rhei.		
Gum Aloes		āā grs. xv.
Pulv. Ipecac.		
Sapo. Hisp.		āā grs. xij.

M. Ft., Pill No. 50. "Two of these pills should be taken twice or three times a day or often enough to keep the bowels *very open*."

Dr. Hale rests the cure chiefly on "tonics, such as lime water and infusion of bark." Dr. Wood says "the most efficient remedies are said to be tonics, antacids and laxatives." Dr. D. S. Brandon, in a paper before referred to, lauds turpentine, combined with castor oil or laudanum, according to the state of the bowels; "say twelve drops three or four times a day, on a little loaf sugar, and in no instance that I know, or have heard of, has it failed."

Dr. Armor proposes the syrup of hypophosphites, combined with Sime's Elixir of Peruvian Bark. It may be surmised

that the Elixir has as much to do with the cure as the hypophosphites.

I am accustomed to use the two following prescriptions with uniform success:

℞—Mag. Calc. ʒ i.
Sapo. Hisp. pulv. grs. x.
Camphor "
Sang. Can. " āā grs. v.
Mix. Dose from three to five grains, four times daily.

℞—Cinch. Rub. ℥ ss.
Ferri. Carb. Prace.
Rad. Rhei. āā ʒ ij.
Port Wine Oj.
Ft. Mist. Dose, table-spoonful with each meal.

When there is loss of appetite I direct the tonic to be taken half an hour before eating, and the powder soon after, otherwise both are to be taken soon after eating. In many cases where there is not much constitutional debility, and in the earlier stages I find the first prescription amply sufficient to control the disease.

Many local applications have been proposed; among them nitrate of silver has received its due amount of laudation. I have seen this used repeatedly, often with no benefit, and at most affording but temporary relief. With correct views of the pathology of the disease, such an event might be anticipated. When I hear of a physician using this treatment I am constrained to think he does not know what he is dealing with—that his opinion of the disease does not extend any further than he can see.

Very many writers and practitioners recommend, in severe cases, the *weaning of the child. My experience and observation is such as to lead me to hestitate to recommend any such procedure.* It may be conservative to the mother's health, but there is a tender offspring, who claims our sympathy and consideration as well as the mother. The chances of rearing children throughout our land are few enough at best—small, indeed, in our cities—and fearful to the child that is weaned in the first weeks of its existence. A recommendation to

wean the child in such cases in our city, is but a warning to prepare its shroud. I scarcely know an instance in this city where a child has survived the first two or three years of its perils, when weaned as a cure of stomatitis. I have seen mother's turn away from such counsel, and avow their resolve to suffer on rather than expose their offspring to such fearful odds. There may be, and indeed are, severe cases when this proceedure seems imperative. I speak only against the indiscriminate recommendation of some practitioners, believing that, with proper treatment, a vast majority of mothers will survive and preserve their offspring.

With the above digression I return to the consideration of the pathology of the disease.

Post mortem examinations of the disease are not abundant. A few have been made. I take from a valuable contribution to the literature of this disease, the following appearances, as described by Dr. McLean:

"The stomach was almost completely denuded of its mucous coat, with numerous patches of ulceration extending deep into its muscular tissue. A small patch around the pyloric orifice of the stomach, was the only healthy portion." Trans. Ind. Med. Society, 1856.

Many writers have asserted positively that the disease occurs only in females, chiefly during gestation and lactation. Others, on the other hand, declare as assuredly that it occurs also in men, boys, and girls. I can only add to the strength of the latter position. I am certain I have seen it in that class of persons with characteristics as prominent and unmistakable as in nursing women. Its only difference seemed to consist in less persistency, owing to a more ready removal of the causes. I had abundant opportunity to examine a case of the kind occurring to a gentleman about thirty years of age. In this case the stomatitis occurred several times, immediately after eating blackberries preserved in tin cans a long time. Each attack was ushered in by the scalding sensation in the mouth, so intense as to cause severe suffering for several days, by vesicles with inflamed bases, by corrugation of the mucous membrane on the side of the tongue, slight swelling and

ulceration of the mucous membrane. In this instance the primary effect was obviously on the stomach. The stomatitis not following immediately, the cause was not discovered till after eating several times.

It may be true that we do not meet with it in so severe a degree as in nursing women. Such might be anticipated. All know that pregnancy and lactation exert a powerful and modifying influence on the stomach, both in health and disease. But I think that here the nervous system is more the medium for communicating sympathetic influence than the blood.

"Its attacks are not confined to any particular constitution or temperament, but are at times made on the most robust, who always enjoyed good health." Bell & Stokes, vol. 1, page 53.

This accords with my experience and observation, but is hardly consistent with the exclusive anœmic hypothesis of some writers.

It does not come within the scope of my object to give a detailed account of the local, mouth symptoms of this malady. They have been already sufficiently indicated in the foregoing. My object has been to indicate a few points in the history and character of this malady. If the position assumed is correct, the mouth symptoms become comparatively unimportant to the practitioner. His remedies must be directed to the stomach. My aim has been to substantiate the following positions.

First. That the history of the disease extends too far back to entitle it to the claim of a new disease.

Second. That so far as known it occurs in all localities and is not limited to malarial districts.

Third. That the primary seat of the malady is in the stomach, modified in the female by gestation and lactation, and followed by constitutional aberration as *sequences.*

This is sustained, 1st, by correlative symptoms; 2d, by therapeutical applications; 3d, by post mortem examinations.

Fourth. That nursing women are the chief sufferers, but that it occurs in other classes of persons.

ARTICLE 4.

ROCKFORD, Ill., March 1860.

Editors of the Chicago Medical Journal,

DEAR SIRS:

In the January number of your Journal I was happy to find Prof. Hudson's "Prize Essay on opium," in its "connected and more intelligible form."

If Prof. Hudson "enters the averment of a *stout denial* to the teachings of the great English author, Pereira," I trust he will pardon an humble member of the profession for offering a *small criticism* to one part of his Essay.

It is this, found on page 41 of the Journal: "a *convenient* and *safe* rule for the administration of opium to infants under a year old, is to give a drop of officianal laudanum for every *month* in the child's age." (The italicizing is mine.)

When I read the above quotation, I supposed it was an "*imperfect print*," but finding no correction in the February number of the Journal, I can understand it *only* as "the text of the undisturbed deliberations of the Essayist," and could not let it pass without notice.

When I was a student I was told Pereira was good authority. Acting upon that teaching I purchased his work, and have ever since been pleased with it. Turning to article Opium, under the head of Tr. Opii, or Laudanum, I find, "for an adult (the dose of laudanum) varies from minims x to f℥ i. To *children* it must be given with the *greatest* caution. I have seen a powerful effect produced in a very young infant by *one* drop." Other authors speak of the *medium* dose of laudanum as *twenty* drops for an adult. *All*, or at least *most* writers agree that infants do *not* bear opium in the same proportional dose, that they do most other remedies, compared with adults. A person 20 years old, may, for practical purposes, be called an adult. Now if an infant twelve months (1 year) old, will "*safely* and *conveniently*" bear twelve drops of laudanum, then a person 20 years old will bear, at least, twenty times as much, two-hundred and forty drops, or f℥ iv, or twelve grains of solid opium. Well, I know adults *will* bear such a dose of opium, for I have administered it. But, that is not

granting it is a "*safe* and *convenient*" dose, meaning, of course, an *ordinary* dose. It would be only in *extreme* cases I would venture upon such *heroic* treatment. On page 39 of Journal I find, "If an infant is tender and susceptible, it is therefore more susceptible to medicines, a responsiveness which holds to the advantage of the discreet physicians. It then requires a less dose," but infants do *not* bear the same proportional dose of laudanum with adults, so I think twelve drops of laudanum to an infant one year old would be equal to f 3 vi, or equal eighteen grains of solid opium to an adult. And on page 41 I find, "The poisonous dose does not appear to be reached *much short* of *twenty grains*. A *few* may be poisoned with a *less* dose, but many will escape the toxic influence of a much larger quantity." Now, if (as I think has been fairly shown) that Essay teaches eighteen grs. of opium a "*safe* and *convenient*" dose for an adult, I think it comes mighty near to a poisonous dose, so near, it should be administered *only* by the most *careful* hand, guarded by a most accurately adjusted balance. It *is* too near a toxic dose to be "*safe* and *convenient*."

Pereira says of the dose of opium, "one-eighth to one-half a grain is a *small dose* for an adult, from half a gr. to two grs. we term a *medium* dose, and from two to five grs. we denominate a *full* or *large dose*."

While *I* would without hesitancy far exceed the dose as given by Pereira as a "*full* or *large* dose" in extreme cases, (and I doubt not *he* would too) I can but depricate the open handedness with which it is dealt in the Essay.

On page 42 a case is reported, as being rather remarkable in which "a child not quite six years old took *seven* and a *half* grs. of opium without a fatal effect." Now if we figure on that case, we find that a child one year old will take *twelve* drops of laudanum as a "*safe* and *convenient*" dose, then a child six years old should be able to take seventy-two drops, or three and three-fourths grs., just one-half a fatal dose, in ordinary cases. But *one-half* of a fatal dose of any remedy will be administered, by the discreet physician, *only* in *extraordinary* cases of sickness.

If you will allow me a little more space in your Journal, I will give my views of the use of opium in two or three diseases, and by these views you will see I highly prize opium, not only as a *palliative* but as a *remedy*. But first I will give my rule as a "*safe* and *convenient*" use of laudanum, for infants. It is, for ordinary cases, one drop of laudanum for every *year* in the child's age, to be doubled in a case of severe suffering. This is my direction to "Mothers," but *I* double and quadruple that dose without hesitancy, in a case of *great* emergency, but *never*, I think, did I give a child one year old, *twelve* drops of laudanum, and I hope I never may feel obliged to.

I think the Professor rates the value of morphine too *low*, compared to opium. I offer it merely as my opinion, that one gr. of morphine is fully equal to four of opium.

Now, as to opium in Dysentery. Eleven or twelve years ago last fall, I concluded opium was good in Dysentery, and determined the next case of that disease I saw, I would give it the full benefit of a *full* dose of *ten grs.* But my *first* case was a feeble female, who had been given to the use of Homœopathy for a number of years. For twenty hours previous to my call she had been up as often as every *half* hour, with a *constant* desire to sit on the stool, very severe bearing-down pain, and the passages of blood and mucus. I gave to her *eight* grs. of opium at *once*, and dealt other powders, of opium v grs., quinine iij gr. each, to be taken every three hours through the night, *if she got up as often*. This was a little after dark, and she did not have to leave the bed again till morning, and then to assist about the house, as she had *no* movement of the bowels for a number of days, she required a laxative. Succeeding so well in the treatment of that case as the *first* on my new plan, (I say, *my*, for I never heard of opium being given so freely, in dysentery, before,) I have followed the same plan invariably, of giving eight to ten grs. at *once*, and in most every case have succeeded as well. A very few times, opium has failed me, and *when* it fails, I am completely at a loss *what* to do. I said I *invariably* followed the opium plan of treatment. I do *not* and with *infants* I *dare not*. Hence dysentery in *infants* is the plague of my life.

And to the use of opium in intermittent and bilious fevers, a word:

I think I can safely say, I have not made, for adults, twelve prescriptions in twelve years, for the arrest of an intermittent, remittent, or bilious fever, when quinine was the base of the prescription, but what opium entered *largely* as one of the *remedies.* I *always* use it, and *rely* upon its beneficial effect. In "congestive intermittents," it, with me, stands at the top of the tower, if seen in the stage of congestion. Illustrating my views by the case of Mrs. S., as reported on page 37, I should have preferred for the first prescription, Opii. gr v, Camphor gr. iij, and Ipecac gr. ij, and as soon as re-action had become well developed, quinine and opium at shorter intervals than recommended by Dr. H. though in *that* case the paroxysm, it seems, was promptly arrested by the administration of the remedy at *long* intervals.

I am surprised that Prof. H. did not speak more particularly of the use of opium in the treatment of Pleurities and Pneumonitis, especially of the last mentioned disease. "Walshe on the Heart and Lungs," goes through the treatment of Pneumonitis without so much as intimating that there is such a remedy in the Materia Medica as opium. If a father saw his little family of half a dozen children sinking for the last time, to find a watery grave, and he could save *one*, and *but one*, and *the* one he chose, he would be placed in straightened circumstances in making a selection; so, in selecting from a half dozen of the remedies for the treatment of pneumonitis. But *I* should say, most *emphatically*, let *opium* be *one* of the remedies.

Hoping I may have offered at least *one* beneficial thought,

I remain, very truly and sincerely, yours,

J. Blount, M. D.

The above criticism, in one point, at least, is justly liable to *criticism*, for 240 drops of laudanum is equivalent to *two* fluid drachms, not *four*, as stated; the writer evidently based his calculation on minims instead of drops.

ARTICLE 5.

TAPE WORMS.

BY E. C. DE PUY, M. D., OF FREEPORT, ILL.

Was called, October 13th, 1859, to see Mrs. S. a pale delicate woman, with a "dark, leaden appearance of the contour of the eyes," aged 29 years. The mother of two children, the youngest five years old. Found her just recovering from an attack of convulsions.

After a careful examination for the cause of the attack, I could not find anything wrong, except some decayed teeth, which were very sore, and had troubled her for some time. The functions of the various organs, digestive tube, lungs, heart, urinary and generative, were apparently healthy. As she complained of a peculiar sensation—epileptic aura—about her jaws, and running from them down the back of the neck, I was led to think the decayed teeth the cause of all her trouble. They were all extracted by her dentist, and she was put upon vegetable bitters and iron to improve her general health. Her appetite improved, and she said she felt better, only sometimes she was very nervous, and that ugly sensation about her jaws and neck troubled her some.

She remained thus until the night of the 25th of February 1860, when she had three attacks of convulsions, which were relieved by Tinct. Gelseminum, Fl. Ext. Scutellaria, Chloric Ether ää in tea-spoonful dose, and Chloric Ether by inhalation; also, friction along the spinal column, with stimulating liniment. On the following morning another careful examination was made to ascertain the cause of the convulsions. The urine was tested for albumen by nitric acid and heat, and found perfectly free—no coagulum. The bowels moved every day and appeared perfectly natural—function of the womb healthy. Ordered Hydrocyanate of Iron ʒ i., Ext. Hyoscyamus ʒ i, Mucilage q. s. to form mass, to be divided into 120 pills. Dose, one pill three times a day, to be increased according to the effect produced; also for her nervousness, Fl. Ext. Scutellaria ℥ ii. Fl. Ext. Pruni. Virginian. ℥ i Simple

Syrup ℥ i. in tea-spoonful doses, as occasion required. Improving, but complains of a constant spitting of offensive mucous, insensibly rising up in her throat, which symptoms, taken in connection with the nervous phenomena, led me to suspect the presence of worms. To operate mildly upon the bowels, and partly as a vermifuge, she was ordered from 3 to 5 grain doses every four hours, of the following compound: Podophillin ʒ i, Pulv. Jamaica Ginger ʒ i, Sugar of Milk ℥ i; mix intimately. Also to take 2 grain doses of Quinine three times a day, for its tonic effect on the nervous system. The powders operated very mildly but effectually upon the bowels, producing very offensive stools, of the consistence of tar, mixed with flattened seybala, representing the appearance of having been retained in bowels for some time; also, what we but little expected, a large number of pieces of tape worm, from a single joint to half a yard in length. The powders were continued, as it was evident that they were doing good in breaking up the habitat of, and otherwise disturbing the worm. The diet was very light—a little panada and wine.

After three or four days of such treatment, thinking that the worm was sufficiently exposed to be operated on by vermifuge or vermicide medicines, she was ordered a wine glassful of the following decoction, to be taken every half hour or every hour, as the stomach would bear, until all was taken: Bark of pomegranate root ℥ ii, boiling water Oii; steep down to one pint. To be followed, if the bowels were not much moved, or the worm not passed, by a dose of castor oil and turpentine. Commenced taking the decoction about noon; at 11 o'clock at night the husband called and said that the decoction was all taken, and the bowels had just moved, and he thought the worm had passed; and also that she was very faint, but free from pain, and otherwise comfortable, and desired to know if he should give the oil and turpentine. She was ordered a little wine and water, and rest until morning, when I visited her, and found that the bowels had just moved again, and that she was very comfortable. The last stool contained no more joints of the worm. The stool which

passed at 11 o'clock contained the tape worms (Tænia Soleum) which I send you for the College Museum. The bowels have moved without purgative medicines since, and contain no more of the tape worm.

The family are "good livers." Have not eaten much pork for years. They are natives of New York State. Have been in this city some four years. The patient now (March 15th, 1860) says she feels better than she has before for years. She is very nervous sometimes, but feels nothing of that drawing of her jaws and neck.

She is now taking the Hydrocyanate of Iron pills, and Tilden's Aromatic Calasaya Wine, as a tonic and a preventive, as worms are supposed not to relish aromatic bitters. It is encouraging for the patient to know that there are fully as many "heads as tails" in the mass of worms which were passed.

ARTICLE 6.

CASES OF RE-SECTION OF THE UPPER JAW,

WITH SUGGESTIONS OF AN IMPROVEMENT IN THE OPERATION OF REMOVAL OF NASO-PHARYNGEAL POLYPI.

BY DANIEL BRAINARD, M. D.

Re-section of the upper jaw is not so rare an operation as to attract very great attention at the present day. It is, however, an operation of recent date, although parts of the jaw and in cases of necrosis, even the whole, were long since taken away. The practicability of re-secting it for disease is said to have been first demonstrated by Gensoul, of Lyons, and its removal first effected by Dupuytren. Even the whole of it, on both sides, has been successfully removed by Maisonneuue, Heyfelder, Tarefi, Lizzars, Liston, and others. In this country, the upper jaw has been removed by Warren, Mott,

Stevens, Eve, Mutter, Rogers, Mussey, Pancoast, Jamison, and probably by many others.

I have removed the whole of the upper jaw, on one side, including the malar and part of the palate bones, three times, and parts of it several times. The first operation was performed May 23, 1846, and published in the medical journals of the time. The tumor was fibrous and cartilaginous, probably not malignant, as the patient died about two years after, without return of the disease. The second will be reported herein.

The circumstances requiring the operation are well known, excepting as regards the case of polypus of the nostril, of large size, and extending into the pharynx, for which the removal of the jaw has been performed, but concerning which the practice cannot be considered settled. Flaubert, Michaux, Robert, Maisonneuue, Davigne, and Arrachart, have resorted to the practice. Dr. Mott laid open the nostril in a similar case, and by cutting away the anterior bony margin of that cavity was able to arrive upon the root of the polypus. Roux also resorted to incision of the nostril, but Gerdy, in the discussion at the Society of Surgery in 1852, questioned the propriety of these operations, and asserted that by repeated attempts naso-pharyngeal polypi can be removed without them. Mr. Nelaton incised the veil of the soft palate in order to arrive upon the root of a polypus.

Prof. Laugenbeck, of Berlin, has lately performed an operation "which consists in re-secting the nasal bones and the orbitar process of the superior maxillary bone, leaving a bridge of periosterum by which they may remain adherent to the neighboring parts and which permits them to re-unite after the polypus has been removed and the bones replaced. Prof. Laugenbeck has performed this operation with perfect success." (*Med. News and Library*, for March, 1860.

M. Legouest incised both the hard and soft palate. *Gazette Des Hopiteaux*, 1859, p. 168.

I add to this paper a note of some cases of this kind in which polypus was removed without incision.

Case 1.—Removal of the upper jaw for cancerous disease:

Caleb Inman, of Wisconsin, a farmer, aged 60 years, consulted me October 17, 1851, on account of a tumor of the left upper jaw. It projected in front so as to elevate the cheek—downward into the mouth where there were openings caused by the falling out of the teeth, from which pus and serum discharged—encroached upon the nostril, from which there was a discharge of mucous and projected externally beneath the zygomatic arch.

The disease had commenced about six months previously by a discharge from the nostril. Soon after a tumor appeared beneath the eye, which ruptured or was opened. Its growth had been rapid and attended with much pain. The general health was much impaired. He had been treated with iodide potass. internally, and applications of nit. silver locally.

The operation for its removal was performed October 18, 1851, in presence of Drs. Rutter, McArthur, and several other physicians and students.

The steps of the operation need not be described, as they were the same in every respect as those described in the case Miss Derey, already referred to.

The points of importance, however, to which I would call attention are these:

1. Uncovering the tumor freely by an incision extending from the middle of the upper lip to near the internal angle of the eye, and another at right angles with this, extending over the zygoma. Where the whole jaw is to be removed these free incisions are required. Where pieces only are to be taken away, it may be done through the mouth alone.

2. Dividing the palate portion of the superior maxillary and palate bones with a metacarpal saw and the nasal process, the connection of the malar to the external angular process, and the zygoma with a pair of strong bone scissors.

3. Driving a chisel, with a mallet, into the temporal fossa, and using it as a lever to start the jaw from its connections. This is only required when the tumor has a certain degree of firmness.

4. Putting upon the cheeks, from the angle of the mouth backward, the forceps with slide for controlling hemor-

Fig. 1.

rhage, already described in this journal, a figure of which is re-produced here, representing the instrument of half size. (Fig. 1.)

I have used these forceps recently, in an operation for the removal of the upper jaw, and in two very severe operations for deformity of the face, and found in all, that it controlled the circulation in the facial artery perfectly.

The attachments of the soft parts can then be readily divided with the bistoury.

This plan permits the greatest dispatch, which, when there is much hemorrhage, as was the case in this instance, is an essential point.

The patient was kept upon stimulants and a nourishing diet and at the end of three weeks was able to return home with the wound nearly healed.

The tumor was soft and the antrum found filled with a fungus growth. For four years there was no return of the disease, but at the end of that time the growth re-appeared, and soon caused his death.

Case 2.—Mrs. Rogers, of Bureau Co., Ill., aged 20 years, was sent to me by Dr. Hopkins, of Princeton, in March, on account of a tumor of the right upper jaw. She stated that about two years since, she had a tooth that was troublesome, and which she dug out herself about a year ago, March, 1859. Soon after, a fungus began to grow from the socket which was repressed in some degree by caustic. This proving insufficient, it was removed by operation, August, 1859, soon after, the fungus re-appeared and was again removed by operation, Nov., 1859, this time the wound cicatrized, and for about three months no return was noticed. In Feb., 1860 it grew again so rapidly, that March 5th it was of the size of a small orange, projecting in form of a bleeding fungus into the mouth. The external appearance is shown in the

figure. This patient is young and vigorous, and her health has suffered but little, although recently she has taken morphia to obtain rest at night.

It was removed March 7, 1860, with the assistance of Drs. Wheeler, Powell, Durham and several students, in the manner already described. The spongy part of the ethmoid bone and the vomer were so connected with the diseased mass as to render their removal advisable.

The tumor was distinctly encephaloid in structure, and under the microscope, presented the appearance described as that of cancer cells, in a marked degree.

No dangerous symptoms followed, and at the end of two weeks the patient was able to walk about and take her usual food.

Case 3. Bony cyst of the anterior wall of the antrum removed:

Mrs. R., aged 34 years, of good constitution, consulted me, in the summer of 1858, on account of a projection of the left cheek, presenting a projection of the size of a large nut. It was of bony hardness, tender to the touch.

She stated that this swelling made its appearance twelve years previously, when it was soft and free from pain. It gradually acquired a bony hardness. In December, 1857, it suddenly became painful, the pain extending over the whole of that side of the face and head. After this had continued about two weeks she felt a sensation of something "giving way," which was followed by a copious discharge of pus from the nostril, which had continued up to this time.

This tumor was removed by an incision through the cheek. A bony cyst of the size of a hickory nut being cut away with the chisel and hammer. In the side next the antrum was an opening through which the pus had discharged

itself. A tent was kept in the wound for three weeks, when it was withdrawn. At the end of six months the wound was healed and the discharge of pus had entirely ceased. Recently, more than a year after the operation the patient remained quite well.

Suggestions Concerning the Removal of Naso-Pharyngeal Polypi, without Removing the Upper Jaw.

1. The treatment of ordinary gelatinous polypi, consists in extracting them carefully and perfectly with the forceps.

2. Fleshy and Fibrous polypi, which hang down behind the veil of the palate, should be seized in the pharynx by strong forceps, and removed by a rotary movement.

3. Vascular polypi may be crushed and left *in situ*, when they disappear.

These propositions will, I think, hardly be questioned, for putting a ligature around a polypus which sinks behind the veil of the palate, is unnecessary; if it remains above it, it is very difficult, and often impossible.

But there remains the question of the treatment of those large, firm polypi, situated deeply at the back part of the nostril, which cannot be drawn forward, and which do not project sufficiently behind the palate to admit of being seized with ordinary forceps, and for which the operation of removal of the upper jaw, an incision of the nostril or of the soft or hard palate, has been proposed and performed by Mr. Manne, in 1847, and by M. Maisonneuue, after what he terms the method of the *boutonniere*, viz: by an opening in, instead of division of the edge of the veil of the palate.

In 1841 I attempted to remove a large fleshy polypus with the forceps, which resisted all my efforts, although it was seized repeatedly, and torn. By passing the finger behind the veil of the palate for the purpose of exploration, which can be readily done, I found a body so large projecting beyond the posterior nasal orifice, into the upper part of the pharynx that it was at once evident that it could never pass through the nostril. I then seized it as firmly as possible with the polypus forceps, and instead of pulling it forward, thrust it backward,

when its attachments were broken, and it was removed through the pharynx, In this case a gush of blood followed, which had not been anticipated, and which, for a moment, threatened to suffocate the patient. Under the stimulus of necessity, I seized a piece of sponge and passed it behind the soft palate, pressing it into the posterior orifice of the nostril, so as to plug it perfectly, drawing it into the nostril with the forceps carried through that cavity.

I have since operated upon two cases of the severest forms of nasal polypi projecting into the pharynx, in a similar manner, viz: by seizing the anterior part with forceps, and pressing backward until the back part could be reached with another and much larger pair of curved forceps, with which it was firmly grasped and torn away by a gradual but firm rotary movement. In one of these cases the polypus immediately after removal weighed eight ounces. The hemorrhage was terrific but was stopped by filling the nostril from behind with pieces of sponge. This method of plugging the nostril seems to be little known, but is not difficult to accomplish in the adult, in the natural state of the parts. When the pharynx has become accustomed to the presence of a polypus, and the soft palate has been pushed downward and forward, nothing can be easier.

This is a point of importance, for the motive which induced the older surgeons to adhere to the ligature, was undoubtedly the fear of hemorrhage. The only danger which need be apprehended from the use of sponge, in this way, is that of its falling into the air passages, as happened in a case reported by Prof. Peaslee, in which, however, the sponge was, I think, introduced from before.

By this means I am sure that some polypi may be safely extracted which otherwise resist all attempts, unless the jaw itself, or the soft or hard palate, or both, be divided. I do not, however, assert that this is always the case, since the extent of the attachment, and its firmness, might offer too great resistance. If this should be found, on trial, there still remains the method resorted to by professor Letenneur, of the School of Medicine of Nantes, who, in a case of this kind,

having failed to pass a ligature around the pedicle, resorted to the use of forceps similar to the *enterotome* of Dupuytren, and by fixing it successively upon different portions with the screw, each in turn was destroyed, the pedicle being at last seized. The dangers incident to the removal of the upper jaw, and even to the incision of the palate by the method of the *boutonniere* are sufficient to induce the surgeon, before proceeding to the application of either of these methods, to use with care all the resources of art for the purpose of effecting his object in a manner less severe. It is probable that more perfect instruments may yet be produced, which will render this object of more easy attainment.

REPORT OF MEETING

OF THE

CHICAGO ACADEMY OF MEDICAL SCIENCES, MARCH 2ND, 1860.

The Academy assembled in their New Rooms No. 95 Washington Street, and being called to order by the President, the proceedings of the last meeting were read, accepted and ordered to be recorded. The Secretary read a report from the Council Committee appointed to make purchase of furniture and fittings for the New Rooms. Submitting, therewith vouchers for moneys expended, which was also accepted, and ordered on file.

Dr. HAMILL read a paper on Sulphate of Quinine and some of its effects, a copy of which the Academy requested for publication.

Dr. INGALS thought the remedy was generally given in too large doses, thereby proving in some cases a local irritant and developing any latent typhoid tendency.

Dr. HAY agreed with the last named gentleman in recommending quinine in small doses frequently repeated in the treatment of intermittent, did not consider it a local irritant.

Dr. RAUCH did not believe quinine to be a local irritant,

but thought that it must necessarily be absorbed in order to produce its physiological effects.

Dr. O. SMITH thought that the remedy in question, must act by means of absorption, upon the nervous system as a stimulant contracting the arteries and increasing the fullness and hardness of the pulse.

Dr. HOLMES asked for information regarding the efficacy of small doses, and the actual quantity necessary to be administered.

Dr. WICKERSHAM thought 14 to 16 grains the smallest quantity necessary to be introduced into the system in order to overcome an attack of the intermittent.

Dr. MACALISTER related the treatment of two cases illustrating the efficacy of small doses of quinine, preceded by a mercurial cathartic after large doses had proved inefficient.

Dr. BYFORD thought that in the cases related by Dr. Macalister, much allowance was to be made for the influence of change of climate and season, and related a summary of the changes which had taken place in the mode of using quinine during a period of twenty years within his experience; he considered the remedy to possess in addition to its other qualities, diaphoretic properties, and favored its administration in large doses—3 to 5 grains. He considered the absence of uric acid in the urine to be attributable rather to the diminution in the waste of tissue than to defective elimination. He believed that quinine was an irritant relatively, frequently as much so in small as in large doses, and that its effects were generally too evanescent to prove injurious. He had seen no harm nor any good result from its use in the treatment of typhoid fever, and believed that in order to the production of its full therapeutic effect, a certain specific quantity was necessary to be introduced into the system which was only to be determined by experience.

Dr. WICKERSHAM considered the remedy decidedly productive of injury to the brain when incautiously administered.

Dr. RAUCH favored the administration of quinine, in small doses, continued for a longer period of time, and thought that the changes in the mode of practice in different localities

might be accounted for by changes in the types of disease. He believed that the remedy was useful in typhoid fever, as a tonic during convalescence, but at no other time during the continuance of that disease.

Dr. Chas. G. Smith considered quinine to be an anti-periodic simply, acting through the medium of the nervous system, and was as appropriate a remedy in one form of periodical disease as in another; hence its efficacy in neuralgia. He did not agree with Headland in his theory that it supplied a deficient element to the blood.

Dr. Bloodgood had commenced in the early portion of his career, with the use of small doses of quinine and had found them entirely successful. Some years afterwards he, in common with other practitioners in the same locality, had found it necessary to increase the doses in order to produce the desired effect. He did not believe the remedy a local irritant, did not approve of small doses, and always waited for complete intermission before using it at all.

Dr. Graham strongly advocated the use of large doses of quinine, had seen many lives sacrificed by the too sparing use of that remedy. He considered it necessary to place the system thoroughly under its effects, and speedily, in the severer form of intermittant, in order to insure the life of the patient.

Dr. Hamill had used carbonate of ammonia, and also prussiate of iron, in conjunction with a diminished portion of quinine, and had found them valuable and efficacious substitutes for a portion of the usual quantity of the alkaloid used.

Dr. Ingals inquired at what periods typhoid fever had been first observed in the West, and in what localities these observations had been made.

Dr. Byford had seen it first in Southern Indiana, in 1843.

Dr. Ingals, in Central Illinois, in 1847.

Dr. Rauch, in Iowa, in 1854.

Dr. Bevan had seen cases of typhoid fever fabricated by the use of quinine, and did not believe that this agent ever orginated or kept up intermittent fever, which idea he thought to be an error originating with Hahnemann and his followers.

The debate was continued by Drs. H. Bloodgood, Graham, Rauch, Orrin Smith, and Ingals.

Dr. HAMILL declined having his paper published.

Reported for the *Chicago Medical Journal*, by

WALTER HAY, M. D., Secretary.

Miscellaneous.

VARIOLA AND VACCINA.

Unsuccessful attempts to produce Variola in the Cow, by Inoculating with the Virus of true Variola. Perfect success on using the ordinary Vaccine Virus. BY EPHRAIM CUTTER, M. D., Woburn, Mass.

Acting upon the commonly received opinion both of the public and the profession—namely, that cow-pox is small-pox modified and mitigated by a transmission through the system of the cow—it would be natural for any physician to expect to procure pure primary vaccine virus by simply introducing into the system of the cow the virus of variola from the human subject.

It was under this conviction that a series of experiments were conducted upon about fifty kine. It is proposed to give an account of the same in the present paper.

Experiment 1.—Nov. 26th, 1859. Inoculated four young kine with variola-virus taken on November 19th, from one of my own patients, at about the eighth day of the eruption. Punctures were made with a lancet upon the hairless skin beneath the tail and near the vulva. The virus was intro-

duced upon quills and covered with isinglass plaster, as in the ordinary mode of vaccinating the human subject. At the expiration of a week, no effect like vaccination was produced. In fact, there seemed to be nothing more than a moderate inflammation, just such as would result from a non-specific puncture.

Experiment 2.—Dec. 27th. With variolus virus taken on the eighth day (Dec. 22d) of the eruption, by Dr. Luther Parks, Jr., of Boston, Dr. Alonzo Chapin, of Winchester, at my request, inoculated five kine. The writer inoculated seven including steers and heifers.

Punctures were made with a lancet, near the vulva or anus, and upon the teats. The quills, charged with the virus, were introduced, allowed to remain a few minutes, and then suffered to drop out. The operations were conducted with the greatest care, so that there should be no mistake. About five quills were used upon each animal in this as in all the other experiments. On the 31st of December a few of the spots presented to the feel a round and flat hardness, about half an inch in diameter. One spot had a central depression. This promised so much that it was very confidently expected that vaccine virus would be obtained. But the hope was illusory, for the spot did not pass through the normal stages of a vaccine pustule. On the contrary, it remained the same for more than a week, and then faded away. It was suggested by Dr. Chapin that the virus might possibly have been taken from a varioloid patient, (it sometimes being very difficult to distinguish between them,) and that thus the experiments proved nugatory. Subsequent experience, however, has not borne out the supposition.

Experiment 3.—Jan. 6th, 1860. Visited a patient of Dr. Toothaker's of Wilmington, sick with severe variola, and charged quills with matter. The eruption was at the seventh day. Inoculated seven kine with this matter, January 6th, 1860. In these cases the cuticle was abraded by scratches, made with a lancet, at right angles to each other, until the serum of the blood began to escape. The charged quill points were then rubbed upon the abrasions for a moment or two

No satisfactory results. To be sure, pustules, or something that looked like pustules, were obtained. They were umbilicated, and some of them hard to the feel, but no lymph could be got.

Experiment 4.—Procured some quills charged with variolous virus, from Dr. R. L. Hodgdon, of West Cambridge, on January 6th, and on January 7th inoculated three young kine with the same. The mode was the same as in experiment 3d. No satisfactory result.

Experiment 5.—Jan. 13th. Visited, with Dr. Drew, of Woburn, a small-pox patient under his care, and charged some quills in the usual way. Besides I charged some cotton threads, by rupturing pustules and imbuing in the lymph the threads for the distance of half an inch or more at their middle part. Within an hour of the procuring of this virus, the quills were inserted into several cattle by the usual punctures with the lancet. The threads were introduced beneath the skin by means of a needle. They were then drawn through to the point charged with the virus, and with this engaged under the cutis, the ends were tied, and the seton thus formed left in. The threads remained in for three days. Inflammatory action ensued. There was swelling, with soreness in the vicinity of the punctures. Upon removing the threads, however, these symptoms subsided. No normal pustule was produced. This essay was deemed almost an "experimentum crucis."

Experiment 6.—Jan. 18th. Went to Lexington and took quills and threads from a small-pox patient of Dr. Currier, in the same manner as was practised in Experiment 5th. These were used in inoculating four cattle on the 20th of January. These essays were without success, although the threads were allowed to remain a week.

Experiment 7.—Jan. 25th. Received by express, from Dr. John A. Lamson, of Boston, some variolous virus from one of the crew of the slave yacht Wanderer. This matter was selected with care, and the case was a well developed one. These quills were used upon four or five cattle.

At the expiration of a week, there was one pustule devel-

oped out of the sixteen or twenty punctures. Took what seemed to be lymph from this one pustule, and tested it, without success, upon another cow.

It is natural to expect, that after so many careful experiments, conducted without succes, the experimenter should begin to doubt. I questioned my ability to inoculate; but of this I was not convinced, as I had successfully vaccinated a considerable number of human subjects during my practice of medicine. Besides, I was told by some, who had tried the same experiments without success, that it could not be done; that cow-pox must be found in a natural condition.

About this time, Dr. Currier, of Lexington, in conversation with my father, Dr. B. Cutter, stated that within the past ten years he had seen some cases of the cow-pox, occurring in the natural way upon the cow; and he further expressed his belief that such cases must exist in our vicinity at the present time. We should look for them in young cows, just after their second calving, and for the locality of the eruption upon the udder, between the hind teats. One case he mentioned as having about one hundred pock upon a remarkably clear udder. He took a large number of quills from them, and vaccinated three persons with them, viz: himself and two "Irish" infants. He did not succeed in his own case, but the children "took" severely. The remainder of the quills were mislaid, and he lost the opportunity of any further trials.

I was led to examine quite a number of cattle, in different localities, to find this disease in the natural state. I was incidentally surprised to find that a majority of the cattle examined had upon the teats and udder a considerable variety of pustular and horny skin diseases, so that if it is thought (and such is the opinion of the public, I believe) that *purer virus* can be got from the cow than from the human subject, it is an easy matter to explode this idle theory. While conducting Experiment 7th, I found one cow with the hinder part of the udder covered with non-umbilicated pustules of the size and feel of a normal vaccine pustule.

Experiment 8.—I took the virus from the most developed of these pustules, and tried it on other cows. No infection

followed, thus proving (if the operation was properly performed) that this was not the true vaccine pustule.

Experiment 9.—A repetition of Experiment 8th, the matter being taken from a similar pustule near the vulva of another cow, and inserted into another animal. No infection resulted.

Experiment 10.—Still another cow was found with a disease upon the udder, simulating the natural disease. This ran its course in about a fortnight. There were pustules, non-umbilicated, full of white lymph. Vaccination with this lymph upon quills was tried upon another cow, and about a week later some of the crusts were rubbed up with water to the consistency of pus, and then pricked into crucial abrasions of the cuticle of a two-days'-old calf, by Dr. Chapin, and of a cow, by myself. These vaccinations have not yet had time to develop themselves. If they do not take, should we not be justified in calling these cases the spurious cow-pox referred to by Jenner?

Experiment 11.—Jan. 20th, 1860. Vaccinated four kine with ordinary vaccine, such as I was using in vaccinating the human subject.

Jan. 24th.—The spots all look as if taking.

Jan. 26th.—On two of the four kine, umbilicated pustules, having, in one instance, a whitish summit, and in other instances being more swollen, with summits less white.

Jan. 27th.—One of the kine has three spots, half an inch in diameter. Bluish color, well marked. Took a large number of quills, and on the same evening sent specimens to the members of the Middlesex East Society, and to other physicians who had assisted in procuring variolous matter for the purpose of experiment.

Jan. 28th.—Dr. Chapin visited the animals. He was assured of the abnormal characteristics of the *quasi* pustules procured by inoculation, and was satisfied with the normal appearance of the pustules produced by the vaccination.

Experiment 12.—Jan. 23d, 1860. Vaccinated two cows with vaccine virus from a child, on the seton plan. Did not take. Failure probably due to the imperfect moistening of the threads.

Experiment 13.—Jan. 24th, 1860. Vaccinated, on the seton plan, four kine, with virus received by mail from Dr. J. D. Mansfield, of South Reading. No other results ensued than what would ordinarily be expected to follow the introduction of an uncharged thread.

Experiment 14.—Jan. 28th. Dr. Chapin vaccinated two kine with the virus he ordinarily uses upon the human subject. Both took well, and a large number of quills were obtained from them, which were used with general success.

Since the last experiments, I have often successfully vaccinated kine, both with the crust and the quill. The pustules have generally been large, and have matured upon the eighth or ninth day after vaccination. They vary in size somewhat, being generally very large, and not small. In some, a characteristic blue color of the pustule and vicinity is observed. This happens especially when the seat of vaccination is upon the part of the labium where the skin merges into the mucous membrane. No constitutional effects upon the cows have been noticed.

From the account given in a late number of the *Boston Medical and Surgical Journal*, of the experiments of Dr. Martin, of Attleborough, Mass., some years ago, and the most unfortunate results that followed, I tremble at the risks I have been running, for if quills had been obtained they would have been used. It is only an instance of an overruling Providence. However, I am more confirmed, by Dr. Martin's account, in my opinion, derived from the above-described experiments, that *vaccinia is not varioloid*, but that it is a *distinct affection*. I cannot explain the experiments of Dr. Adams, of Waltham, Mass., nor of Mr. Ceely, nor of the Russian physician, who profess to have succeeded in inoculating cows, and thus procuring the vaccine disease. I only know that Dr. Chapin and myself did not succeed. If some more successful operator should report and annul my assertions, I shall be very happy, as all that I desire is the truth.

Three modes of introducing the variolous matter into kine were used—(inoculation.)

1. By quill and puncture with lancets.

2. By rubbing the charged points of quills upon crucial abrasions of the hairless cutis.

3. By introducing, in the form of setons, threads charged with the variolous virus. This is the easiest and most expeditious way of inoculating or vaccinating kine. There is but one struggle with the animal, and, once in, it stays in.

Vaccination on the cow has been practised in the following ways:

1. By the seton; this was tried twice and was not successful.

2. By quills. These, if fresh, generally succeeded.

3. By pricking into crucial abrasions of the cuticle, with a lancet, portions of a scab dissolved in water, until it is of the consistence of a thick paste. This has been uniformly successful upon man or beast; more so than any other mode I have practised.

I think I am justified in asserting that *any one can procure a vaccine pustule on the cow by vaccination*, as easily as it can be procured upon the human subject. This is the mode in which I obtain "*vaccine virus* from the cow."

The question has been asked whether the virus from the cow, thus obtained, is any better than the ordinary virus in use. To this I would reply, than in my opinion it is no better. A few statistics may throw light upon this. Out of nine primary vaccinations noted, with virus from the cow, six took the first time and three the second. Out of fifty-three secondary vaccinations noted, thirty-nine took the first time, thirteen the second time, and one the third time. One of the primary vaccinations taking on the second trial, occurred in a child half an hour old. One of the secondary vaccinations taking on the first trial, occurred in a lady 91 years old, who had been repeatedly vaccinated unsuccessfully. Another of these was vaccinated during the prodomic symptoms of varioloid, and both vaccinia and varioloid ran through their stages contemporaneously.

On the other hand, the writer vaccinated a child with quills from another infant, and they did not take. On the second trial, quills charged with virus from the cow were used. No effect. On the third trial, the third mode of vaccinating, with

a scab from the cow, was employed. No result. On the fourth and final trial, the same mode of vaccinating, with a scab from a *child*, succeeded perfectly.

Some of the secondary vaccinations with virus from the cow procured pustules normal in appearance, and of a large size, despite a good scar of the primary vaccination. Constitutional effects have been produced in some cases. However, equally good pustules and similar constitutional effects have been produced, at the same time, by the ordinary virus in use.

Another thing—the ease with which the vaccine pustule is produced in the cow ought to give the profession confidence *in the matter in general use.* Thus the idea that it has "run out" by successive transmissions through human subjects is not supported by the present experiments.

Again, the idea that the vaccine disease is peculiar only to certain districts in Wales is not founded upon fact, as it has been repeatedly observed in this country. It seems idle, then, to send to Europe for vaccine virus, when we have it at our very doors.

To conclude: The object of the present paper has been to show that vaccinia is not varioloid, or cow-pox modified small-pox. It has been attempted to prove this by the unsuccessful attempts to produce a normal vaccine pustule by inoculation, *while upon the very same animals, by vaccination with the virus in ordinary use*, the normal vaccine vesicle has been got easily. According to the authorities consulted, it is still a mooted question in regard to the subject in hand. But Von Bibra says distinctly that the cow-pox and the small-pox are two different diseases. The asserted fact that persons who have had the small-pox have been successfully vaccinated, seems also to substantiate our position.—*Boston Medical and Surgical Journal.*

DO BAD SMELLS CAUSE DISEASE?

The tendency of the human mind to rest satisfied with any belief that is authoritatively asserted, is too well known to require any comment. Philosophers of all kinds are no more exempt than other people from this easy style of dealing with difficult problems. Medicine is, we think, especially chargeable with cherishing pet answers to questions that force themselves unkindly on her; and we think that the way in which she has made up her mind as to the causes of various kinds of fevers is an example of this style of cutting the Gordian knot.

Of late years, it must have struck all our readers that pig-styes, dirty pools of water, open privies, ash-heaps, etc., have been declared highly criminal, and on all occasions even adjudged guilty of producing any kind of fever or bowel complaint that may have broken out in the neighborhood. If a child happen to suffer typhus in a farmhouse, it is the mixen at the end of the barton that caused it. If an epidemic of English cholera befall a village, it is traced to the duck-pond by the road-side. If in a wealthy household the inmates are stricken with diphtheria, some open sewer close at hand, has, as a matter of course, been the cause. So accustomed are we to hear this sort of reasoning resorted to on all occasions, that one feels a little difficulty in expressing doubts as to the certainty with which the effect is unhesitatingly traced to its cause. Nevertheless, we think there is at least sufficient evidence to cause reflecting minds to pause ere they give in their adhesion to the general opinion, and thus shut their eyes to further research and inquiry. Dr. Watson, has, we know, stated it as his distinct opinion, "*that neither animal nor vegetable decomposition is sufficient to generate fever of any kind;*" and the researches of Dr. Guy, and other observers, have certainly gone some way to support that opinion.

Dr. Guy, in his very interesting contribution to the *Journal of the Statistical Society*, on the health of Nightmen, Scavengers, and Dustmen, gives us a mass of statistical facts which, it must be confessed, run counter to the generally received opinion,

that foul animal or vegetable emanations are the fruitful source of disease. This class of men, without doubt, spend their days in the very midst of filth of all kinds. He says:

"In most of the laystalls or dustmen's yards, every species of refuse matter is collected and deposited—night-soil, the decomposing refuse of markets, the sweepings of narrow streets and courts, the sour-smelling grains from breweries, the surface-soil of the thoroughfares, and the ashes from the houses."

This heterogeneous mass the scavengers or "hill" people have to sort or to pass through sieves, so that the emanations arising therefrom must be brought into intimate relation with their lungs and skin. If fever and diarrhœa are so clearly traceable to the vicinity of these so-called noxious materials, surely the scavengers ought to be a poor, fever-stricken race. A medical examination, however, of this class of workmen as compared with brickmakers and bricklayers' laborers, proves that the scavenger is comparatively exempt from disease. Thus, among a number of men examined in each of the three classes, it appeared that the numbers attacked by fever were, among the scavengers, 8 per cent.; among the bricklayers' laborers, 35.5 per cent.; and among brickmakers, 21.5 per cent.

This result seems extraordinary enough; but it may be argued that these men do not live in the laystalls or dustyards, and therefore that their exemption from fever may be attributable to this; but what can be said if the master dustmen and their families, who live all their lives in the midst of these heaps of so-called fever-nests, are healthy. Dr. Guy says:

"I do not think that, whether in town or country, such another body of men (as master dustmen) could be brought together, except by selection; and it is not going too far to assert of them that, if the comparison were limited to the inhabitants of London, or our large towns, no score of selected tradesmen could be found to match the same number of scavengers brought casually together."

Unless we suppose that the scavengers get used to this so-called miasmatic atmosphere, or that after a time it no longer affects them, we cannot see how the foul emanation theory

can hold water. Nature cannot work in one place differently from another. Night-soil must be just as deadly in an open yard in London as in the country. But here we have the experiment tried on a larger scale, of a whole class of men subjected to foul emanations, and yet they are far from being an unhealthy race, and are not nearly so prone to fever or bowel disease as the brickmakers' laborers.

We are far from wishing it to be understood, however, that we do not consider foul emanations as dangerous or baneful under any circumstances. In our opinion, they become noxious when much concentrated. Our houses, for instance, are built on the principle of a bell-glass; and our drains and privies, and all other impurities, if allowed to give off a deleterious miasma, most certainly do become most virulent sources of disease. But, in the open air, we think it very doubtful whether these emanations are ever the cause of injury to man.

Let us watch with Dr. McWilliam a still more gigantic experiment on the health of the Thames waterside people, which has been going on for years, and is still proceeding. The whole sewerage of two millions and a half of people, has, within the last ten years, been turned into the metropolitan stream. Year by year its waters have become more contaminated, and its smell more disgusting. It should follow that the health of the waterside community is proportionally decreasing; that febrile complaints, cholera, and diarrhœa, are alarmingly on the advance. But what is the real state of the case? Dr. McWilliam, in his Report for the year 1858, on the health of the Water Guard and Waterside Officers of Her Majesty's Customs, says:

"As respects bowel affections, in which I include diarrhœa, choleraic diarrhœa, dysentery, etc., the types of those forms of disease, which in this country noxious exhalations are commonly supposed to originate, we find the additions during the four hot months of the past year from this class of complaints 26.3 below the average of the corresponding period of the three previous years, and 73 less than those of 1857."

The quantity of putrescent animal and vegetable matter in

the Thames has been going on increasing; but the illness generally attributed to the emanations arising therefrom has been decreasing! We know that many will urge that all the combustibles (if we may use the term) being thus accumulated, it only requires the match to be applied, to find epidemics raging like wildfire. But the year before last, cholera did break out on the banks of the Lea, and there died out, apparently from want of sustenance. This year, according to the *Lancet*, cholera, veritable Asiatic cholera, has been on board the Dreadnaught; yet it has not spread, and there seems no likehood of its doing so for this season at least. As Dr. McWilliam truly says, "It is nowhere sustained by evidence that the stench from the river or docks, however noisome, was in any way productive of disease." It is true that one waterman, in June last, was said to have died of Asiatic cholera, and that his death was ascribed to river-poison; but, as the eminent observer whom we have just quoted, correctly remarks, "it is opposed to all analogy, and to the usual order of nature, and therefore unphilosophical, to suppose that a cause so extensively diffused should have been so singularly limited in its effect."

Greatly doubting, as we do, the alleged ill effects of foul emanations in the open air upon human life, we nevertheless do not think that the crusade against filth should for one moment be relaxed. A bad smell may be no more unhealthy than a bad taste; but we should, if possible, avoid the one as much as the other. What we should, above all things, avoid, however, is the falling into the error of supposing that bad smells are the indubitable sources of many puzzling diseases, and of thus hardening our minds against investigations of the kind which were instituted a year or two ago by Dr. Barker, and which, when completely carried out, will enable us to decide what the noxious principles are which make all the difference between an unpleasant and a malarious odor.

Editorial.

BOOK AND PAMPHLET NOTICES.

A Medico-Legal Treatise on Malpractice and Medical Evidence; Comprising the Elements of Medical Jurisprudence. By John Elwell, M. D., Member of the Cleveland Bar. New York and Cleveland: Alfred Elwell & Co., 1860. Pp. 588. From the publishers.

This book appears to be a full and perfect treatise on the subjects of which it professes to treat.

In how far it is correct in the rules which it lays down, we are unable, from a personal examination, to say. It has, however, received the endorsement of some of the most eminent legal men, as well as of physicians, in this country, and from a cursory examination it appears to be full, accurate, and prepared with care.

Such a book should be in the hands of physicians generally, who are too often not aware of the nature of the responsibility which they assume in accepting the charge of surgical cases.

A Practical Treatise on Operative Dentistry. By J. Taft, Professor of Operative Dentistry, in the Ohio College of Dental Surgery. With eighty illustrations. Philadelphia: Lindsay & Blakistone. Chicago: Jones & White, Dental Depot.

The author of this book seems less anxious to be regarded as the originator of the theories he adopts, and the practice he recommends, than to give a well-digested and systematized *resume* of the theories of dental diseases, as adopted by the

best Dental Pathologists, and the practice prevailing among the better class of dentist in treating these diseases.

In a brief but comprehensive "Introductory," he describes the varies kinds of *tartar* that deposits upon the teeth, and the best method of removing it; the nature, cause, and effect of atrophy, exostosis, denuding of the enamel, chemical abrasion, necrosis of the teeth, &c., &c.

He then takes up and describes very minutely, the characteristics and causes of "caries of the teeth."

The causes are considered under two general divisions, Predisposing and Exciting. Among the former are hereditary defects—functional derangements, which prevent a proper elimination or appropriation of materials necessary for the perfect structure of the tooth—vitiated secretions of the mouth—badly performed dental operations, and the like.

Among the most active of the exciting causes, all decomposition of animal and vegetable matter between and around the teeth—contact of hydrochloric acid with the teeth, consequent upon the eructation of the fluids of the stomach of dyspeptics—medical preparations, and condiments containing acids—acids formed by the decomposition of substances in the mouth, in consequence of a constant galvanic action going on between different metals used in filling natural teeth and plates for artificial teeth. The author then passes on rapidly, making some valuable suggestions in regard to the treatment of superficial or slight caries of the teeth, and comes to the important subject of *filling teeth.*

In discussing the merits and demerits of the various materials used for this purpose, gold and tin are considered to be the only ones of much real value. Amalgams or cements, which must always contain mercury, and are usually made by mixing mercury, tin, and silver, together, are very properly regarded as "*vile compounds*, never to be used as fillings for the teeth.

Then follow descriptions and cuts of instruments necessary and proper for preparing cavities and introducing fillings—forming of cavities, and the proper method of introducing and finishing fillings—treatment of exposed nerves—setting

pivot teeth—necessary instruments for, and the method of using them in extracting the various classes of teeth and roots—treatment of undue hemorrhage after the extraction of teeth, etc., etc.

It will be seen from this somewhat detailed, though quite imperfect summary of contents, that this work is just what its title indicates. "*A Practical Treatise on Operative Dentistry.*"

Dr. Taft evinces an intimate and practical knowledge of what he is writing about, and in all cases uses such simple and well chosen language that there is no mistaking the idea he wishes to convey—making this one of the most valuable books ever published for the use of students of Dental Surgery. A.

I. The Diagnosis, Pathology, and Treatment of Diseases of the Chest. By W. W. Gerhard, M. D., one of the Physicians to the Pennsylvania Hospital, etc. 4th edition, revised and enlarged. Philadelphia: Lippincott & Co.; 1860. Pp. 448. From S. C. Griggs & Co.

II. A Practical Treatise on the Diagnosis, Pathology and Treatment of Diseases of the Heart. By Austin Flint, M. D., Professor of Clinical Medicine, etc., in the New Orleans School of Medicine, etc. Philadelphia: Blanchard & Lea, 1859. Pp. 473. From Keen & Lee.

The work of Dr. Gerhard has been long before the public, and its character is now well established.

The present edition, however, contains about one hundred pages of new matter, and many additions relating to Pneumonia, Pulmonary Consumption, and Diseases of the Heart. The work is concise, accurate, and well adapted to supply the wants of the student and the practitioner. We extract passages referring to the use of cod liver oil and the phosphates, which are of particular interest:

"The following may be given as the results of the use of cod liver oil in the medical wards of Pennsylvania Hospital during the last six months, (1849–50:)

1st. *Of the Oil.*—That the light-colored oil can be taken without difficulty by patients whose stomachs have steadily rejected the brown oil.

2d. *Of the mode of administration.*—That a few of the patients have taken the oil without any adjunct to disguise its taste. That its nauseating properties are corrected by its administration with milk; but that its taste is most effectually disguised by the froth of porter.

3d. *Of the time of its administration.*—As a general rule it has been taken before meals, but in four instances where it was not tolerated before meals it was readily taken after meals.

4th. *Of diarrhœa as a contra-indication to its use.*—The existence of diarrhœa is not a positive contra-indication to its use. In three instances in which patients were thus affected, no increase of the symptoms was produced by its use, and no diminution by its abandonment. In a fourth instance, when the diarrhœa had previously existed, the discharge appeared to be increased by the exhibition of the oil, and abated with its withdrawal.

5th. *Of its effects in cases of phthisis pulmonalis.*—Patients using the oil have increased in flesh, in weight, and strength. While using the oil, their cough and expectoration have diminished; that with some, hectic and rigors have entirely disappeared. Six of them have been so much benefited as to leave the hospital and resume their former occupations. In one instance, a patient who entered the hospital with cough, copious purulent expectoration, extreme emaciation, inability to leave his bed, and with the physical signs of a cavity under the left clavicle, after six months use of the oil, left the hospital weighing 140 pounds, with little or no cough, no hectic or rigors, and with an almost entire absence of expectoration, the physical signs having greatly diminished.

6th. *Of the physical signs.*—The improvement of the physical signs is not coincident with that of the general symptoms.

7th. *Of its use in general scrofula.*—In scrofulous diseases where there was no reason to suspect the existence of pulmo-

nary tubercles, the improvement of the patient's health has been very decided.

8th. *Of congestion of lungs as produced by cod liver oil.*—There has been no decided evidence of such a result following the use of the oil in the preceding cases. [Two patients of the twenty, while using the oil, had severe attacks of hæmoptysis, but there was no reason to refer them to the use of the remedy.]

9th. In those cases which have terminated fatally, the appetite, the nutrition and strength of the patient appeared for a time to be decidedly increased; the life of the patient seemed to be in this manner, temporarily protracted; but for a few weeks immediately preceding death the remedy seemed to have entirely lost its value.

10th. *Length of time, etc.*—To be of any decided permanent benefit, its use must be steadily persisted in. It should be continued even after the most striking symptoms of the disease have, in a great measure disappeared.

* * * * * * *

Another mode of treating phithisis which has attracted much notice within the last few years is the use of different phosphatic salts, given in connection with one another. These are the phosphates of iron, soda, lime, and potash. This plan was proposed by Dr. Churchill, and was founded on the supposition that the addition of these salts to the blood would enable it to withstand the tendency to tuberculous deposit. I have been chiefly in the habit of giving these salts simply mixed with water, directing the patient to shake it up before taking it, so as to suspend the phosphate of iron, which is of course insoluble. The mixture is made according to the following formula:

℞.—Sodæ phosphat, ʒiv; Calsis phosphat, ʒij; Potassæ phosphat, ʒi; Ferri phosphat, ℈ij; Aq. Rosæ, ʒ vi.—M.

S. A teaspoonful after breakfast and dinner, first shaking the bottle. If the substance is well shaken, the phosphate of iron may be of course sufficiently suspended to be taken in a proper dose.

Mucilage should not be added, as it would soon spoil. My

friend Dr. Jackson has been is the habit of giving the same medicine in sirup, making a more uniform and prettier preparation, but one which I do not like so well as a simple suspension of these salts. If, however, this preparation should be preferred, the best mode of making it is one given by Mr. Parrish in the Am. Joun. of Pharmacy for 1857, vol. xxix. p. 572.

The use of these phosphates became very general in Philadelphia, but has much diminished within the past year or two. This comparative disuse of them has arisen simply from the doubt that remained in the minds of those who employed them as to their utility."

The volume of Dr. Flint is new. Its scope and object are sufficiently shown by the following extract from the preface:

"As regards the order in which the different diseases are considered, the plan usually pursued may be said to be synthetical, inflammatory affections being taken up first, and afterward the lesions which are, to a considerable extent, the results of inflammation. A method which may be distinguished as analytical, has appeared to the author preferable. Pursuing this method, the work commences with the consideration of organic affections. Enlargement of the heart, occurring often consecutively to other lesions, takes precedence. To this subject the first chapter is devoted. Lesions affecting the walls of the heart naturally come next in order. These constitute the subject of the second chapter. Valvular lesions are then considered, occupying two chapters, and a chapter is devoted to congenital malformations. Several affections which are incidental to diseases of the heart, are treated of in a distinct chapter. Then follow the inflammatory affections, and, afterward, functional disorders of the heart, three chapters being allotted to these classes of disease. Finally, thoracic aneurisms, which claim considerations in connection with diseases of the heart, are made the subject of the concluding chapter."

In regard to the merits of the work we have no hesitation in pronouncing it full, accurate, and judicious. Considering the present state of science, such a work was much needed. It should be in the hands of every practitioner.

Without intending to enter into the discussion of any points concerning which a difference of opinion might exist, we extract our author's views on the hygienic and medical treatment of certain valvular lesions, which we deem of importance.

"Undue excitement and overtasking of the heart induce weakness and favor dilatation. The muscular power here, as in other situations, is exhausted by too great exertion, and the walls yield more readily to distension, under these circumstances, from the accumulation of blood within the cavities. The causes, already referred to, which excite unduly and overtask the heart, viz: excessive muscular exercise, mental excitement, the intemperate use of alcoholic stimulants, etc., are, therefore, to be avoided with respect to the second, not less than the first object of treatment. Exercise, however, within certain limits is, highly important with a view to the preservation of the power of the heart's action. Patients affected with obstructive or regurgitant lesions, will retain a compensatory vigor of the heart, and the epoch when dilatation succeeds hypertrophy will be postponed for a longer period, by habits which involve a judicious amount of exercise, than by a life of complete repose. Active occupations, whether pursued as a calling or for amusement, or with reference merely to exercise, should not be abandoned. Persons under the necessity of performing daily manual labor will do better to continue to work, so far as they are able without inconvenience, than to become fixtures in the wards of a hospital. They who are above this necessity should either follow some active pursuits, or engage in sports which demand a certain amount of physical activity. Indolence or inaction of the muscular system tends to produce weakness of the heart and favors fatty degeneration, thereby contributing to the production of dilatation rather than hypertrophy. The rules which should govern exercise have already been considered in connection with the treatment of hypertrophy, to which the reader is referred.* These rules are applicable to cases of valvular lesions, with or without hypertrophy.

* Chap. I, page 72.

They are, of course, not to the same extent applicable to cases in which the lesions have already led to dilatation.

The diet suited to obviate a tendency to weakness and dilatation is that best adapted to healthy nutrition. Healthy nutrition, and thereby the muscular vigor of the heart, require blood rich in nutritive materials. A poor and insufficient diet tends to hasten the evils resulting from valvular lesions. The diet should embrace a fair proportion of animal food. Liquids should be taken sparingly, the object being to secure a good quality, but not to increase the quantity of blood. Restrictions, as respects fatty substances and those readily converted into fat, are important, if there are grounds to suspect a disposition to fatty degeneration. The articles of food should be adapted to the digestive powers. The action of the heart, as is well known, is liable to be disturbed through its sympathetic connection with the stomach, when digestion is labored or imperfect. Dyspeptic disorders will claim appropriate treatment. Tonics and stimulants, in moderate quantity, are indicated whenever the digestive powers are enfeebled. Exercise in the open air, within proper limits, is important with reference to its influence on digestion. Cheerfulness and mental recreation are desirable for the same end.

Opposite conditions of the blood alike tend to weakness and dilatation, viz: plethora and anæmia. If the blood be too abundant, and the red globules in excess, the heart is overtasked and unduly stimulated. Bloodletting, under these circumstances, may be appropriate. But it should be employed with discrimination and great circumspection, inasmuch as the impoverishment caused by its injudicious employment is a condition worse than plethora. In general, other methods of depletion, which are not spoliative, are to be preferred, viz: saline laxatives and diuretics, in conjunction with a dry diet. Anæmia is a far more unfavorable condition than plethora, and claims efficient treatment with chalybeate tonics, nutritious diet, etc. The symptoms referable to the heart, in some cases of valvular lesions, are, in a great measure, due to functional disorder incident to anæmia, and when the anæmic

condition is removed, all the symptoms may disappear. This fact should be borne in mind. The practitioner is liable to consider all the symptoms as resulting directly and exclusively from the lesions, and, consequently, is led to exaggerate the immediate danger from the latter. Patients who suffer much from palpitation, etc., when anæmia is conjoined with valvular lesions, may experience no inconvenience when the blood is restored to its normal condition. This is intelligible in view of the well-known fact that anæmia often gives rise to functional disorder of the heart when this organ is free from organic disease."

NEW MEDICAL JOURNALS.

THE CINCINNATI MEDICAL AND SURGICAL NEWS, Edited by A. H. BAKER, M. D.

This is what was the *Cincinnati Medical News*, now appearing in pamphlet form, with change of name, and is a very creditable publication of 32 pages, issued monthly.

THE SAN FRANCISCO MEDICAL PRESS, Edited by E. S. COOPER, A. M. M. P.

The *Press*, the second medical journal emanating from the city of San Francisco, is a quarterly publication, containing 64 pages of printed matter, and is furnished to subscribers at two dollars per annum, payable in advance.

CHILBLAINS — MR. DUCHESNE-DUPARC'S FORMULA. — Mr. Duchesne-Duparc has kindly forwarded to us his receipt for chilblains, with some remarks on the treatment of this distressing affection:

℞ Liq. calcis, 4 oz.; liquoris ammoniæ, 1 ʒ; spirit. minth. 4 ʒ; tinct. saponis, ʒ 1. M. For embrocation night and morning, on the parts affected.—*Champonniere's Journal.*

Typhoid Fever—Nathan Smith, &c.—Dr. Bowling, of the *Nashville Journal*, for March, is anything but "genial." The good humored comments of "A," upon his proposed treatment of typhoid fever, have given rise to an exacerbation of unpleasant emotions alarming in the last degree. For the use of the term "inventor," which he re-produces in capitals, and garnishes with an exclamation point—

> "Which stands in state, with vacant wonder fraught,
> The pompous tombstone of a pauper thought."

We confess to have relied upon Ainsworth, Webster, and Johnson. It appears that our amiable critic relies for his definition upon the Patent Office—does he propose to take his medicines from or to the same quarter? We yield to no man in admiration for the talents and acquirements of Nathan Smith. He needs no eulogium from "A," neither from the senior editor of the *Nashville Journal*, although that senior editor claims peculiar powers of "writing up," whoever will bow to his behests. *Does Dr. Bowling understand?*

As a matter of history, every New Englander will recollect that from the great use of that particular article (buttermilk) by Dr. Smith, as an article of diet and diluent, in typhoid fever, his practice was familiarly termed "the buttermilk practice," by both opponents and friends.

We have yet to learn that it is necessary to endorse every particular of doctrine and practice adopted by any eminent medical gentleman, in order to show that we worthily recognize his merit. In all humility we confess that we do not even do this for the senior editor of the *Nashville Journal*.

A.

☞ The American Medical Association will hold its Thirteenth Annual Meeting at New Haven, on the first Tuesday of June, 1860. The Secretaries of Local Societies, Colleges, and Hospitals, are requested to forward the names of delegates as soon as they are appointed, to

Stephen G. Hubbard, M. D., Secretary,
New Haven, Ct.

THE ILLINOIS STATE MEDICAL SOCIETY AND THE NATIONAL ASSOCIATION.—At the last meeting of the Illinois State Medical Society a movement was made, looking to *reform* in the manner of electing the President of the National Association. A resolution relating to this matter was introduced, as was understood, at the instance of Dr. N. S. Davis, but did not pass. At present the Association has the right of choosing its officers from any part of the Union, although for obvious and proper reasons the Presidents have most frequently been taken from the State in which the meeting was held. Dr. Davis proposes to make a rule prohibiting such choice, and disfranchising the members of the profession in the particular State or city in which the meeting may be held, and urges his friends to rally and put it through.

This is an ingenious move. In the first place it might make his chances of election better at New Haven than they were at Washington and at Louisville, where he did not succeed. Secondly, if the Association should ever meet in this State it would prevent the election of any other person. If this resolution passes, the next move may be to invite the Association to Chicago, otherwise not.

It is melancholy to see so much attention paid to an unimportant matter, to the neglect of the scientific interest of the State and National Societies. But it is in accordance with the peculiar system of elevating the profession advocated in a certain set just at present.

For ourselves, we cannot but express a hope that our friends will, whenever consistent with their interests, attend these meetings, and that they will take pains to arrange, for the purpose of being communicated, any valuable facts or observations which may have occurred to them during the year. Thus interest may be added to the meetings, and value to the transactions.

☞ The annual meeting of the Illinois State Medical Society will take place on the third Tuesday of May, 1860.

MILWAUKEE, March 13, 1860.

To the Editors of the Chicago Medical Journal:

GENTLEMEN: I notice in the last number of the *Medical Examiner*, printed in your city that E. B. Woolcott, of this city, attempts to defend himself against the imputation resting upon him, of malpractice in the case of Page, the fireman injured at the Watertown accident, by raising a question of "medical ethics."

According to his own showing, he left Page as he found him. If it was a fracture why did he put on the mechanical power, and why did he leave it without the usual dressing?

If it was a dislocation, why did he not reduce it?

In regard to the question of ethics it is doubtful what claim Dr. Wolcott may have on the regular profession to be recognized. If, as is generally understood, he at one time left its ranks, and announced himself a disciple of Turner, author of the notorious book called, "Fallacies of the Faculty, and the "Chrono-Thermal System of Medicine," which he will hardly dare to deny—and if he has never publicly renounced this *ism*, it is but just that he should be called upon to *purge himself* of this contempt before appearing to urge a charge of violation of the code of ethics. The certificate of character volunteered by the *Examiner* is not sufficient. Let the Doctor himself say where he stands just now.

Respectfully,

MEDICUS.

☞ Any of our subscribers having a copy of "Todd's Cyclopedia of Anatomy and Physiology," can find a purchaser, if desired, by addressing the editors of the *Journal*.

☞ We are obliged to omit, until the next number, the names of the graduating class of Rush Medical College for the session of 1859–60.